

Before dealing with the special form of turbine most suitable for marine purposes, the author gives a condensed and useful account of the development of the turbine from the time of Hero to the present day. In the second chapter the main features in the design of marine turbines are discussed, and then a detailed account is given of the method of blading the turbine drum and casing. In a convenient table at the end of this chapter the author gives in detail the sizes of the blades and their spacing for the H.P., L.P., and astern turbines of an ocean liner. The next chapter is well illustrated, and the reproductions of photographs, taken at various stages in the process of building a large set of marine turbines, will do much to make the non-technical reader familiar with the more important details in turbine construction. The thermodynamic principles which govern the design of turbines are then briefly touched upon, and their application illustrated by the calculations, necessary in determining blade dimensions for a channel steamer, being fully worked out. The success of the marine turbine is so entirely dependent on the efficiency of the propeller to which it is connected that it was most desirable that Prof. Biles should in these lectures discuss fully the theory and design of the screw propeller as employed in turbine vessels; the important and complete series of experiments carried out at the United States Navy Tank at Washington are admirably summarised; the results are illustrated by graphs, and their application to the design of a propeller which has to work under any given set of conditions is clearly explained.

The book concludes with a summary of all the trustworthy information at present available as to the comparative economy of turbine and reciprocating marine engines, and it is worth pointing out that, judging from the performances of certain cross-channel steamers, the considerable economy of the turbine-engined steamer which is shown during the preliminary trials is apparently not maintained in active service, though the author has every confidence that this loss in economy, which he considers is due to cavitation, will eventually be overcome.

T. H. B.

*A First Year's Course in Geometry and Physics.* By Ernest Young. Parts i. and ii. Pp. viii+101. (London: George Bell and Sons, 1907.) Price 1s. 6d.

THIS satisfactory first year's course of work in geometry and physics recognises fully the desirability of making the introductory lessons in mathematics and science as practical as possible. The author is an experienced schoolmaster who understands the need of setting young pupils to do things for themselves if they are really to understand the subjects under consideration. Though there is little that is novel in the methods adopted, the book provides an abundance of well graduated exercises suitable for boys of twelve to thirteen years old.

*New Geometry Papers.* By Rupert Deakin. Pp. 103. (London: Macmillan and Co., Ltd., 1907.) Price 1s.

THE recent changes in teaching geometry have rendered Mr. Deakin's "Rider Papers on Euclid" of little use in most schools, and the present book is intended to serve a similar purpose under the new conditions. The papers are graduated and arranged in order of difficulty, while hints are provided on the method of solving riders. The collection should prove useful as a supplement to the school text-book of geometry.

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## LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

### The Name of the Cave Horse.

IN the paper on the derivation of the horse in the April number of the *Quarterly Review*, to which reference was recently made in NATURE, Prof. J. C. Ewart expresses the opinion that the wild Mongolian horse, commonly known as Przewalski's horse, is identical with the horse depicted by the prehistoric inhabitants of La Madelaine Cave, in the Dordogne. The same view is even more emphatically expressed by Dr. E. L. Trouessart in the Bulletin of the Paris Museum (1906, p. 453). No mention is made by either writer of any change in nomenclature involved in this identification.

In the Phil. Trans. for 1906 Sir R. Owen described the prehistoric horse of the cavern of Bruniquel (Tarn et Garonne) as *Equus spelaeus* and although he estimated the height of the animal at 13½ hands, it is practically certain that it was really much less, as he was not aware of the relatively large size of the molars in the Mongolian horse.

Now it can scarcely be doubted that the small prehistoric horse of the Bruniquel cave is identical with the one depicted by the prehistoric hunters of La Madelaine, and as there is equally little doubt that both are merely races of *Equus caballus*, the name of the La Madelaine horse is *Equus caballus spelaeus*.

Hence, if Messrs. Ewart and Trouessart are right, it is also the title of the wild Mongolian horse, the name *Equus przewalskii* dating only from 1881. It is true that there is a possibility that a name given by Hamilton Smith to a horse supposed to be from Tataria may antedate both *spelaeus* and *przewalskii*, while *Equus fossilis*, dating from 1836 or 1846, may possibly be identical with *spelaeus*. Nothing certain can, however, be affirmed with regard to either of these points.

The name *E. c. spelaeus* for the La Madelaine horse certainly cannot be ignored, but it seems inadvisable that it should be made to supersede *E. c. przewalskii*, especially when the wide separation in time and space between the types of the two is taken into consideration. A way out of the difficulty may be found in a refusal to admit Messrs. Ewart and Trouessart's absolute identification of the fossil with the living form, and to regard them as distinct, although closely related, races, when they will stand respectively as *Equus caballus spelaeus* and *Equus caballus przewalskii*.

I may add that I cannot agree with Prof. Ewart in regarding the skull of Przewalski's horse as conforming to the type in which the face is bent down on the cranial axis. On the contrary, it is more or less of the straight type, in common with the prehistoric horses of Europe. The bent type seems to me characteristic of domesticated breeds, especially thoroughbreds and Arabs, and as it also occurs in the fossil Indian *Equus sivalensis*, my suggestion that Arabs and thoroughbreds are descended from that species, while the ordinary "cold-blooded" European horses trace their origin to the "Przewalski," is strongly confirmed. In a short time I hope to put skulls of the two types on exhibition in the Natural History Museum, so that visitors can judge for themselves on the point at issue.

R. LYDEKKER.

### The Enigma of Life.

IN the review of my book "The Evolution of Life" in NATURE of May 2 (p. 1), "J. A. T." admits his inability to "point out precisely where my experiments are fallacious," and says he does "not know what to answer unless it be that the sterilisation was inadequate, or that the preparations were contaminated before the photographs were taken." But the adequacy of the sterilisation, in accordance with all existing knowledge, is fully shown in the book; and, as for the last suggestion, it seems

really absurd when "J. A. T." was told that the organisms removed from the tube were received on a sterilised slip, covered with a sterilised cover-glass, and were there photographed almost as soon as they were found.

That he should not be convinced, however, by my "final decisive experiments" is only what might be expected when he says, in excuse for not himself repeating my simple experiments, "we regard archebiosis as so great a miracle that we do not expect to see it repeated," thus implying a disbelief even in its occurrence in the past. I certainly could not hope to convince anyone, by my experiments, who disbelieves in the natural origin of living matter on this earth when its crust became sufficiently cool to permit of such an occurrence.

Then, "harking back to heterogenesis," "J. A. T." refers to my belief in the origin of *Otostoma* from the *Hydatina* egg, and it is what he says on this subject that tempts me, in the main, to write this letter.

He says I was good enough to show him "the mummy of an *Otostoma* reposing within the egg-case of *Hydatina*. There can be no doubt about it." These latter words, which I have italicised, are of some significance in reference to previous doubts expressed by many persons; but in the former statement Prof. J. Arthur Thomson (for your reviewer evidently affects no concealment of his identity) has certainly said too little. I showed him, not one specimen of *Otostoma* only, but about fifty specimens of this rare ciliate, either within egg-cases of *Hydatina* or lying among them. As he says, he thought it a result of parasitism, notwithstanding all the evidence against this view; and he left me with the expressed intention of investigating the subject himself. He now says that he, and also Dr. John Rennie, "watched many ova of *Hydatina*. But neither the expected nor the unexpected happened." From which I deem it quite possible, judging from the great rarity of *Otostoma*, that neither of them may even have seen one of these ciliates among the *Hydatina* eggs which they were watching. Yet I have taken some hundreds of *Otostomata* from my experimental pots. One may be permitted to smile at the puerile suggestion that, because Dr. John Rennie saw two infusorians moving within a split *Hydatina* egg-case, that kind of thing, which may be commonly enough seen, can at all explain my repeated observations with details and photographs concerning the origin of *Otostoma* from the egg of the rotifer. Yet it is with such a suggestion that "J. A. T." dismisses the subject.

Still, his attitude in regard to this question is much the same as it is concerning archebiosis, seeing that he has previously said concerning it (*NATURE*, February 25, 1904):—"There are some things that one must see for oneself, and even then one would not believe them." I, however, have seen this transformation, marvellous as it is, on so many occasions that I find no possible room for doubt as to the reality of its occurrence. Parasitism, I maintain, is out of the question, because no minute germs of ciliates are known; because of the extreme rarity of this particular great ciliate; and, above all, because it is the whole substance of the egg which becomes transformed within the unruptured egg-case, and because no movement can be seen until the whole mass begins slowly to revolve and speedily issues as a great embryo ciliate—which in its free state attains a bulk two or three times as great. Full evidence in support of this is to be found in my work "The Nature and Origin of Living Matter," chapter xiii., and in the Proceedings of the Royal Society, vol. lxxvi., B, pp. 385-392.

H. CHARLTON BASTIAN.

#### Radium and Geology.

IN his letter in *NATURE* of May 9 (p. 31) the Rev. O. Fisher raises a point of wide interest, but one which admits at the present moment of little more than the suggestion of fresh hypotheses and the destructive criticism of old ones.

Sediments rich in radium involve *prima facie* parent rocks capable of supplying the necessary uranium. Failing this explanation, we must, I think, assume that the uranium is derived from an extra-terrestrial source. Neither hypothesis is at the present moment capable of

proof. Much will turn upon (among other things) our final estimates of the quantities to be accounted for.

With the concluding portion of Mr. Fisher's letter I regret I am not able to concur. Many predictions, based on the best knowledge available, were made of the temperatures which would probably be encountered in boring the Simplon Tunnel. Geologists and engineers both arrived at results much below those which were afterwards observed. That of Heim was 38°-39°. That of Stockalpen (formerly head engineer of the north boring, St. Gothard) was 38°, &c. The highest predicted temperature—then criticised as quite excessive—was that of the "Irfgeneur-Geolog" Stapff, which was 47° C., but the actual temperature reached was 55° C. (see papers by Ed. Sulzer-Zeigler and by Prof. H. Schardt in the *Verhandlungen der schweizerischen naturforschenden Gesellschaft*, July-August, 1904). This will give a considerably higher gradient than that reckoned by Mr. Fisher, more especially as the highest temperatures were by no means coincident with the greatest overlying mass.

Nor do I think the facts will admit of explanation by hot springs coming from below. Prof. H. Schardt, perhaps the highest authority on the subject, in a contribution to the journal cited above, states that the region of highest temperature was characterised by abnormal dryness of the rocks, and to this fact (the absence of circulating water) and the horizontality of the strata he ascribes the specially elevated temperature.

Mr. Fox, in the article in *NATURE* of October 27, 1904 (vol. lxx., p. 628), to which Mr. Fisher refers, states that the ordinary gradient of 1° F. in 70 feet is insufficient to account for the great heat, and suggests a volcanic source.

Trinity College, Dublin, May 13.

J. JOLY.

#### The Relationship of Lemurs and Apes.

IN *NATURE* of May 2 Dr. Elliot Smith has referred to a memoir presented by me to the Zoological Society on "Recently Discovered Sub-fossil Primates" from Madagascar. On the evidence supplied by the brain-casts of three of these fossils, Dr. Elliot Smith takes exception to my conclusion that certain of these extinct Prosimiæ are in many respects intermediate between the extant Malagasy lemurs and the true monkeys.

I have nowhere in my memoir maintained, as Dr. Elliot Smith seems to imply, that, *so far as their brain-conformation is concerned*, these recently discovered sub-fossil lemurs form a distinct link between the existing genera and monkeys. On the contrary, I have emphasised the fact, insisted on by Dr. Elliot Smith himself, that many of them show evidence of marked retrogressive changes in their brain-structure; and I have pointed out that, just because of this *retrogressive specialisation*, it is the condition of the brain which, of all characters, is least likely to afford satisfactory evidence of close affinity between the Malagasy lemurs and the Old and New World monkeys. I will not anticipate the suggestions which I make in my memoir as to the possible causes which have brought about this curiously degenerate condition of the brain of these Malagasy lemurs, nor is it possible here to give in detail the facts and arguments on which I base my conclusion that, *in spite of this brain degeneracy*, these recently discovered fossils do, in fact, afford strong evidence that they, in common with their extant allies, are descended from ape-like ancestors. A detailed study of these fossils and a comparison with their nearest living relatives, on the one hand, and with various genera of Old and New World monkeys on the other, has convinced me that most of the so-called *lemuroid* characters of the extant Malagasy lemurs have been secondarily acquired, and that, taken as a group, the characters which differentiate the Malagasy lemurs, recent and extinct, from the monkeys are so few and (with the possible exception of brain-structure) so unimportant as not to justify their retention in a separate suborder.

A satisfactory discussion of the subject seems, however, hardly possible until the publication of the two memoirs by Dr. Elliot Smith and myself describing in detail the fossils themselves.

H. F. STANDING.

South Kensington, May 9.